young friend to Sir Henry De la Beche, the Director-General of the Geological Survey, that a post was at once found for him on the staff of the Survey, and before many days Ramsay was at work at Tenby. He joined the service in the spring of 1841, immediately after the publication of the little volume on Arran, which embodied the fruits of his labours in previous years. From that time onward his life was spent continuously in the work of the Survey until he retired at the end of 1881. So capable a lieutenant did he prove himself to the chief of the staff, that after only four years he was appointed Local Director for Great Britain.

From the first Ramsay showed that, with habits of patient observation and cautious induction, he combined a faculty for bold and broad generalization. His remarkable paper on the denudation of South Wales, published in 1846, was one of the earliest essays in which the amount and effects of denudation were worked out from detailed surveys of the geological structure of the ground. He then struck the key-note which may be heard through nearly all his subsequent contributions to scientific literature. He was one of the earliest observers to realize that the existing topography of the land has a long and interesting history, much of which may still be deciphered by the use of geological investigation.

The name of A. C. Ramsay will ever be honourably associated with the story of the gradual working out of the records of the Ice Age. Following up the results obtained by Agassiz, Buckland, Darwin, and others in this country, he threw himself with all his ardour into the study of the glaciation of Wales, tracing the limits of the glaciers of that region, and extending his experience by frequent excursions among the Swiss Alps. His scattered papers in scientific journals undoubtedly did much to stimulate general interest in the history of the Glacial Period, and to create a special and voluminous literature of this subject. His views differed much from those of some of the older geologists of the day, and led to some active controversy. Especially did opposition arise when, after studying long and carefully the erosive action of land-ice, he came to the conclusion that certain lake-basins in various parts of the world had been scooped out by ice. Murchison, Lyell, and others of less fame, entered the lists against him; but he had a considerable following among the younger geologists. find this controversy still fitfully continues.

In connection with his glacial work, mention should be made of his bold endeavour to prove that ice-action had been in operation more than once in the geological past. His paper on the Permian breccias of England called attention to the evidence of transport of fragments of rock from Wales, and to the resemblance between these fragments and those in glacial moraines and boulder-clay. He subsequently detected what he thought to be similar traces of ice-carried materials in the Old Red Sandstone; and in one of the last papers which he wrote he gathered together the various pieces of evidence in favour of a long succession of Glacial periods in the geological past.

Two of the most suggestive essays he ever wrote were his well-known Presidential Addresses to the Geological Society in 1863 and 1864, in which he worked out, from his wide practical acquaintance with the stratified formations of Britain, the idea of breaks in the succession of organic remains in the geological record. To the geologist and the palæontologist these papers marked a distinct epoch in the advance of geological inquiry; while to the biologist concerned with the history of the evolution of organized existence on this planet they were full of luminous thought.

By far the largest part of Sir Andrew's contributions to geological literature is to be found in the maps, sections, and memoirs of the Geological Survey. The mapping of the volcanic districts of North Wales, in which he took the leading part, will ever remain the best monument of his skill as a field-geologist. His exhaustive memoir on that region has long since taken its place as one of the standard works of reference in our geological literature.

In his later years he seems to have taken pleasure in reverting to some of the inquiries which he started in an early part of his career. He returned with renewed zest to the study of the history of topographical features, discoursed as to how Anglesey became an island, and traced out the story of the River Dee. In successive editions of the work on the "Physical Geography and Geology of Great Britain," which at first was given as six lectures to an audience of working men, he worked out in greater fulness the chief stages through which the surface of this country seemed to him to have passed before it acquired its present features.

Of the value of his scientific labours full recognition was made by his contemporaries. He was elected President of the Geological Society in 1862, President of the Geological Section of the British Association in 1866 and again in 1881, and President of the Association itself in 1880, when the meeting was held at Swansea. He received the Wollaston Medal of the Geological Society, the Neill Medal of the Royal Society of Edinburgh, and a Royal Medal from the Royal Society. He was chosen into the honorary list of many learned Societies at home and abroad. On the death of Sir Roderick Murchison in 1871, he was appointed Director-General of the Geological Survey. At the end of 1881 he resigned this office, was knighted for his distinguished services, and soon thereafter went to reside at Beaumaris, where his strength has gradually given way, until he died on the evening of the 9th inst.

There was in Sir Andrew Ramsay such simplicity and frankness that men of the most diverse natures were attracted to him, and as they came to know him more intimately the gaiety and kind-heartedness of his disposition attached them to him in the closest friendship. Fond of literature, and glad to relieve the pressure of his scientific work by excursions into the literary field, he had acquired a range of knowledge and of taste which gave a special interest to his conversation. Now and then he found time to write an article for the *Saturday Review* in which this literary side of his nature would find scope for its exercise. But the daily grind of the official treadmill left him all too little time for such diversions. His death removes from our midst one of the foremost geologists of our day, and from the friends who knew him in his prime, a large-hearted, lovable man, whose memory they will cherish till they too pass away.

A. G.

ON VAN DER WAALS'S ISOTHERMAL EQUATION.[1]

ONE of the objections raised against this equation by Prof. Tait in NATURE, vol. xliv. pp. 546 and 627, brings clearly to light the importance of the question whether the finite size of the particles should be accounted for by an equation of the form—

$$p_1(v - b) = \tfrac{1}{3}\Sigma mu^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where p_1 represents the internal pressure, equal to the sum of the external pression p, and the molecular pressure a/v^2, and b some multiple of the total volume b_1 of the particles; or if this equation must rather have the form—

$$p_1 v = \tfrac{1}{3}\Sigma mu^2\left(1 + \frac{b}{v}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

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[1] Prof. Korteweg's paper and accompanying letter are of date November 4, but owing to an accidental delay they did not reach me until after the appearance of my last communication (NATURE, November 26, p. 80). Otherwise I should, of course, have made reference to them. It will be seen that Prof. Korteweg draws attention to the form of the virial equation applicable in one dimension. RAYLEIGH.
December 2.

In the *first* case, in substituting $p + a/v^2$ for p_1, and $R(1 + at)$ for $\frac{1}{3}\Sigma mu^2$, the well-known formula of Prof. Van der Waals is arrived at. In the *second* case, the same substitution leads to a quite worthless formula, unfit to explain even qualitatively the conduct of gases under compression.

The *first* form is the one which presents itself most naturally when, as was done by Van der Waals, the extension of the molecules is considered as a diminution of the volume in which they are moving; the *second* is obtained as a first approximation, when the virial equation is extended to the repulsive forces which come into play at the collisions. Of course, both methods, if they could be worked out with absolute rigour, would give the *same* result; but, this being impossible for both of them, the question as to which gives the better approximation is not at all an unreal one.

Now, it is extremely improbable that this question should have to be answered in a different way for linear and for three-dimensional space; yet for linear space the first method leads to a quite easy and *absolutely* rigorous solution, and the equation thus obtained is analogous to the *first* form.

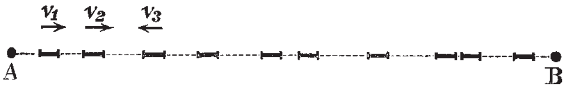

FIG. 1.

In order to prove this, let A B (Fig. 1) represent a linear space of length l, bounded by two rigid walls, A and B, and let there be moving in this space some perfectly elastic particles, all of the same mass, m, and length, λ, but having different velocities, $v_1, v_2, \ldots v_n$. At every encounter of these particles there will be simply an exchange between their velocities; therefore at every moment one of the particles will have the velocity v_1. On this particle we fix our attention, following it on its way till the next collision. After this collision we leave it to its fate, directing our attention to the other particle, which has now acquired the velocity v_1. Proceeding in this manner, it is obvious that at every collision a distance λ is economized, which has not to be travelled over by the centres of the molecules. Starting, then, from the wall A and passing over to the wall B and back again, the number of collisions is $2(n - 1)$, and the distance economized $2n\lambda$, (adding 2λ for the collisions against the walls). The distance travelled over by the centres consequently being $2l - 2n\lambda$, it is clear that the number of collisions with velocity v_1 against one of the walls amounts to $\frac{v_1}{2(L - n\lambda)}$ in one unit of time, and the corresponding change of momentum to $\frac{mv_1^2}{L - n\lambda}$, so that the pressure on the wall is measured by

$$p = \frac{\Sigma mv^2}{L - n\lambda}. \qquad (3)$$

Of course, for space of two and three dimensions, the problem is much more complicated. Yet in 1877 I gave a solution[1] of it for spherical particles which, according to my opinion, is rigorous so far as the several encounters between the molecules may be looked at as independent of one another. For a short time after each collision the possibilities of fresh collisions are considerably influenced by the proximity of the departing molecule. This influence, certainly of very difficult mathematical treatment, is disregarded in my calculations.

The outcome of these calculations[2] is that of every unit of distance only the $(1 - 4b_1/v)$th part has to be travelled over by the centres of the molecules, the $4b_1/v$th part being economized at the collisions. Therefore the number of the encounters with the bounding walls is augmented in the proportion $1 : 1 - 4b_1/v$, and the formula $pv = \frac{1}{3}\Sigma mu^2$ is to be changed to

$$p(v - 4b_1) = \tfrac{1}{3}\Sigma mu^2. \qquad (4)$$

In 1881, my friend H. A. Lorentz applied the virial equation to the calculation of the influence of the size of the particles on the pressure. In this manner he obtained the formula—

$$pv = \tfrac{1}{3}\Sigma mu^2 \cdot \left(1 + \frac{4b_1}{v}\right). \qquad (5)$$

His paper, published in *Wiedemann's Annalen*, Bd. xii. p. 127, was inserted in the German and English versions of Van der Waals's pamphlet "On the Continuity of the Liquid and Gaseous States of Matter," at the end of the sixth chapter.

Considered as a determination of the factor, with which the total volume b_1 of the particles, when introduced in $v - b$, is to be multiplied, our results were identical, and confirmed the opinion expressed by Van der Waals about the value of this coefficient. Mr. Lorentz viewed his results in no other light, and had no intention at all to substitute his formula (5) for that given by Van der Waals. Indeed, in the passage of his paper which I quote here, he clearly indicates the weak point of his calculation:—"Strictly speaking, a correction ought to be made here, indicated by Mr. Van der Waals; in calculating the number of encounters, the extension[1] of the molecules should have been taken into account. The matter is simplified, however, if the influence of the virial arising from the repulsive forces, or the size of the molecules, is small; and if a correction to the first order is sufficient, then the uncorrected value of the number of encounters may be used in calculating the small repulsive virial."

Now it is not impossible to apply to Lorentz's formula the correction alluded to in this passage. In 1875 I calculated for the first time,[2] by a more rigorous method, the shortening of the mean free path of spherical particles, in consequence of their extension in the direction of motion. Some months later, Mr. Van der Vaals succeeded in the same calculations by a somewhat different method, extending it to the case of two sets of particle of different diameters. Both calculations lead to the same result, viz. that the mean free path is shortened in the proportion $v : v - 4b_1$; therefore the number of the collisions, and the term in the virial equation dependent upon these collisions, must be augmented in the reciprocal proportion; but then this equation takes the form—

$$pv = \tfrac{1}{3}\Sigma mu^2\left(1 + \frac{4b_1}{v} \cdot \frac{v}{v - 4b_1}\right) = \tfrac{1}{3}\Sigma mu^2 \cdot \frac{v}{v - 4b_1}, \qquad (6)$$

and becomes identical with the equation (3) of Van der Waals.[3]

In this manner the true formula is obtained by means of the virial equation, as it has been by the method of economized distances, and these verifications of the equation derived by Van der Waals are not without importance. Indeed, I always held the opinion that it is not quite allowable to conclude directly from the diminution of the free path of the molecule to a proportional augmentation of the pressure on the bounding walls. The number of the mutual encounters of the molecules, and the number of their collisions with the walls (or, rather, their passages through an ideal plane), are not proportional numbers under *all* circumstances. A change in the shape of the molecules, or an augmentation of their diameters, will affect the first number in a much greater proportion than the second. But, as I have shown, the equation of Van der Waals holds good, independently of this assumption.

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[1] *Verslagen en Mededeelingen der Kon. Ak. u. Amsterdam*, 2e Reeks, Deel x.; *Archives Neerlandaises*, t. xii.

[2] I am bound to acknowledge that the same correction, which is indicated further on for Lorentz's calculations, has to be applied to the number of collisions given in my paper.

[1] The extension in the direction of the motion is meant here. I have translated the first phrase from the original paper in *Wied. Ann.*, where it runs: "Streng genommen müsste man also hier eine Correction anbringen, wie sie von Herrn van der Waals angegeben würde; man hätte nämlich bei der Stosszahl die Grösse der Molecüle zu berücksichtigen."

[2] *Verslagen en Mededeelingen*, 2 Reeks, Deel x.; *Archives Neerlandaises*, t. xii.

[3] I owe this remark to a verbal communication by Van der Waals.

D. T. Korteweg.

Amsterdam, November 4.

THE BIRD-GALLERY IN THE BRITISH MUSEUM.

A LONG-NEEDED and much-wished-for reform, to which the attention of naturalists should be specially invited, has been commenced in the Bird-gallery of the British Museum. Under the old *régime* at Bloomsbury, the rule was, as it is even now in most of the Continental Museums, that every specimen should be stuffed, and exhibited in the public gallery. The natural, if not the necessary, consequence of such a rule is that, as time progresses, the shelves become crowded with badly mounted specimens, which are very unpleasing to the general observer, and most inconvenient to the scientific worker.

In the British Museum, however, the idea of mounting every specimen has been long ago abandoned. The main collection for scientific work is, we need hardly say, that of skins. These are arranged in cabinets, in numbers which it would be impossible to find space for if "mounted." When thus disposed of they are much more easy to find, and more convenient for examination, than "mounted" specimens. Though it may be sometimes necessary to refer to the Bird-gallery, the working ornithologist of this country, as a rule, uses only the skin collection.

This being so, the question arises as to what is the best way of making the Bird-gallery useful, and attractive to the general public. As to this there can be no question, it appears, that the Bird-gallery should be fitted up as an "Index Museum," and should contain a series of the principal types of bird-life arranged in systematic order from the highest to the lowest. Every family should be placed in a separate case, in its proper position between the two groups to which it is most nearly allied. In each family a series of well-mounted specimens should illustrate the principal sub-families and genera, and the male and female and other plumage of the leading species. Nests and eggs should be added to show the mode of nidification, and maps to show the areas of distribution. Diagrams and preparations of particular structures should be placed at the head of each group, to exhibit its special peculiarities; and finally, every specimen and diagram should be clearly labelled and explained. It will readily be understood that a Bird-gallery filled up in this way would be a most instructive object, and much more useful and attractive than the crowded rows of uniformly-set-up specimens that are offered to view in most public Museums. Some such plan as this, we take it, is what the authorities of the British Museum have now in view.

For a commencement, the family of Woodpeckers has been selected, and a case devoted to its illustration has been fitted up. A series of well-mounted specimens shows the leading forms of the group, and diagrams, preparations, and maps exhibit its principal peculiarities and the distribution of the species.

This is at present only the beginning of a very important change of plan. But there can be no question that if the scheme is carried out, and the whole Bird-gallery is treated in a similar way, an admirable reform will have been effected.

THE OCTOBER ERUPTION NORTH-WEST OF PANTELLERIA.

SOME time after the news arrived in this country of the volcanic outburst in the neighbourhood of Pantelleria, my friend Mr. Gerard Butler, F.G.S., undertook to visit the island, and to investigate the interesting phenomena that were being exhibited there. Mr. Butler has now returned, having made a large collection of specimens of rocks and minerals, and I trust that before long we shall have fuller information concerning this remarkable district. The following short note embodies the general results of his inquiries concerning the recent eruptions; but telegrams received since his return state that renewed outbursts have led to the formation of an island at the spot, and mariners have been warned to avoid it.

John W. Judd.

Royal College of Science, London, December 14.

In Nature of December 3 (p. 120), a short sketch is given of a paper by M. Ricco on the above, which those interested in the subject may read in the *Comptes rendus* for November 25.

It may be worth while for one who visited Pantelleria soon after the eruption to point out that there appears to be no foundation for the idea conveyed by many English accounts and by the words "island," "erupted island," in Nature (*loc. cit.*), that an island comparable to Graham's Island was formed.

It seems that by a submarine eruption which, after prefatory earthquakes between October 14 and 17, was first observed on the latter day, about 5 kilometres to the north-west of Pantelleria, a narrow band of floating bombs, extending for about a kilometre in a north-east and south-west direction, was produced.

The persistence during the eruption of this linear band may perhaps indicate the line of fracture of the sea bottom.

There appears to have been always deep water at the scene of eruption. Ricco tells us that soundings at the middle and ends of the floating shoal of bombs found no bottom at 320 metres.

The brittle cindery bombs readily broke up, giving vent to the superheated steam they contained; when, or upon their becoming otherwise waterlogged, they sank, so that, on October 26, soon after the eruptive action ceased, all traces of it had disappeared in deep water.

G. W. Butler.

NOTES.

We regret to have to record the death of Prof. Stas, the eminent Belgian chemist. He died at the age of seventy-eight.

At last Thursday's meeting of the Royal Society, the President read from the chair a letter from Prof. Dewar, which had been put into his hand as he entered the meeting-room, in which Prof. Dewar stated that he had at 3 p.m. that afternoon "placed a quantity of liquid oxygen in the state of rapid ebullition in air (and therefore at a temperature of −181° C.) between the poles of the historic Faraday magnet in a cup-shaped piece of rock salt (which is not moistened by liquid oxygen and therefore keeps it in the spheroidal state)," and to his surprise, Prof. Dewar saw the liquid oxygen, as soon as the magnet was stimulated, "suddenly leap up to the poles and remain there permanently attracted until it evaporated."

According to information sent to Berlin, Emin Pasha and Dr. Stuhlmann, travelling in the region between Lakes Victoria, Tanganyika, and Albert Edward, have discovered what they take to be the ultimate source of the Nile. This is a river called Kifu, which is supposed to have its sources in the Uhha country, lying to the east of the northern part of Lake Tanganyika,